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SCIENCE AT THE ROYAL ACADEMY.

IN art exhibitions, such as those at Burlington House, in the Grosvenor Gallery, and the like, there are always many paintings so hideous in subject or in treatment, that it may be questioned whether the pain they cause to the artistic eye does not go far to counterbalance the pleasure which the better works are calculated to produce. This is the case even in great national collections, where only the finest works are supposed to be gathered together. There is not a room in the Louvre where there are not paintings and sculptures absolutely painful to contemplate, and annoying even when we try, so far as possible, not to see them. But in annual exhibitions of the works of a great number of living artists, the hideous paintings and sculptures sometimes preponderate to such an extent that a visit to such collections is an actual punishment. Last year, for instance, there were so many bad paintings, and some of them so very bad, so many ill-chosen subjects, and some of them so very faithfully (and therefore disgustingly*) rendered, that the many beautiful works of art which were exhibited were quite insufficient to counteract the painful impression produced by the others, and one found oneself wondering whether the worst of the rejected pictures could have been very much worse than some which were exhibited.

This year the general impression produced by the pictures and sculptures at the Royal Academy is far pleasanter. There are several very bad works, and a few which are positively execrable; but they do not destroy the pleasurable effect produced by a general survey, nor are they so obtrusive as to disturb the artistic mind by their mere presence, during the careful study of the better works.

In these columns, however, we do not intend to consider chiefly the artistic beauty of the various works exhibited at the Royal Academy. Our purpose rather is to note where artists have either failed from want of scientific knowledge, or have availed themselves of such knowledge (or of the close observation of nature which is the basis of scientific knowledge), to produce effects, the truth and beauty of which are at once recognised, even by those who do not understand the secret of the artist's success.

It may be urged at the outset, and is, indeed, often urged by artists themselves, that they represent what they see, and that it is not for others to question the scientific accuracy of this or that portion of a painting or sculpture, when they have not before them the landscape or model from which the artist worked. The truth, however, rather is that the artist endeavours more or less successfully, according to his skill, to represent what he sees. Even where what he is attempting to represent is unchanging, he often fails in his attempt to represent it correctly. But in many cases, the artist is obliged by the very nature of his work to represent not what he sees, but what he has seen—some attitude or expression necessarily fleeting, some aspect of nature necessarily lasting for too short a time to be reproduced save from memory. From a want of knowledge, or from failure to make sufficiently careful observations, the artist may overlook some essential characteristics of what he wishes to represent. He may combine incongruous elements in the delineation of facial or bodily expressions, he may represent a natural feature true enough in itself, in combination with another equally true in itself, which could not possibly be seen at one and the same time as the other. He may not, to use Macaulay's illustration, "mix August and January in one landscape"; but the same sort of reasoning may apply to his less glaring incongruities which Macaulay applied in the other case, and Horace, earlier in the passage which every schoolboy (as Macaulay would say) knows by heart. "Would it be a sufficient defence of such a picture to say that every part was exquisitely coloured, that the green hedges, the apple-trees loaded with fruit, the waggons reeling under the yellow sheaves, and the sun-burned reapers wiping their foreheads were very fine, and that the ice and the boys sliding were also very fine?" It is no better defence to say of Mr. Pettie's picture of Monmouth before James II. (30, Room I.), that from the knee downwards Monmouth's left leg is well drawn, while the rest of the body is, perhaps, placed as it might be if a man with bound arms tried to wriggle along the floor, when it is absolutely impossible that, with the body so placed, the lower part of the left leg could be seen as it is, unless it had been twisted round by main force through some forty-five degrees round the axis of the limb, to the dislocation of bones and the rending of muscles. To take another example from another department of painting:—The blue sea is charmingly represented in Mr. Brett's picture, "The Grey of the Morning" (506, Room V.). We have seen the sea as blue as that, and though it is not quite so level as the sea usually is (Mr. Brett's seas seldom are), the effect considered in itself is very pleasant. Again, the lower cumulus clouds, showing through the grey mist, are well presented, and in effective contrast with the "lily white clouds," which have "got up early and peeped over the wall." But neither Mr. Brett nor any one else has ever, except, perhaps, in a dream, seen that rich blue sea in the grey of the morning. These two characteristic features of his chief painting this year are charmingly represented, and in two different pictures would have been admirable, but in one and the same painting they are not admirable at all.

In figure painting, as in sculpture, anatomy is the science which has first to be considered in estimating the truthfulness of the artist's work. Every sculptor *must* thoroughly study anatomy, not the anatomy of the body at rest only, but of the body in motion; and every painter of the human figure should do so, for the eye alone cannot be

* The "Fishmonger's Stall," in 1879 was an "awful example."

trusted. If we begin our examination of the works at the Royal Academy with reference to this point, we find, even in some of the works of the best artists, errors which are difficult to account for. For instance, in Sir F. Leighton's charming "Day Dreams" (56, Room I), the outer carpal bone of the right hand is incorrectly represented. We do not say that it is not often so shaped. On the contrary, the deformity, for such it unquestionably is (though a slight one), is common enough. Possibly Sir F. Leighton's model, however beautiful in other respects, had a wrist thus malformed; but there was no occasion to reproduce the defect in an ideal painting. In his "Wedded" (71, same room), the lady's unhappy expression seems justified by the exceedingly unsatisfactory anatomical development of her young spouse, the muscles of whose limbs are without depth or fulness, and little more than surface markings. His more pretentious "Phryne at Eleusis" (307, Room III.) is open to anatomical exception, especially with regard to the lower extremities. But the artistic objections to this painting are much more serious than the scientific ones. If Phryne had really so charming a face, but so matronly a form, and flesh so strangely coloured, she would never, unless singularly unwise, have resorted to the expedient she actually adopted to disarm her judges at Eleusis. We take it the real Phryne relied on the perfect harmony of her proportions, the perfect delicacy and tenderness of every outline and every tint, to plead in her favour, not on fulness of development, or the mere evidence of a fine constitution (apart from a most abnormal complexion). Sir F. Leighton's Phryne, however, would have found her judges merciful if she had been content to let them assume that her figure was as perfect as her face.

(To be continued.)

OLD MAY-DAY.

THE day on which these lines appear—May 12—is Old May-day. We must not forget this in reading what our early poets say of the charms of May. The month of May has been shifted since the days of those poets. When Chaucer, for example, spoke of

> the sixth morrow of May,
> Which May had painted with his softë show'rs,
> This garden full of leavës and of flow'rs,

he meant the time of year corresponding to our 14th, not to our May 6. And again, in choosing May for the name of the naughty lady whom Sir January took to wife, Chaucer was assigning emblematically to May 9 the qualities described by the author of the "Menagiana." The reader will remember that Budgell, in the *Spectator* (No. 365), referring to this description of the kindly warmth infused into earth and its inhabitants during the month of May, expressed the opinion that "the observation is as well calculated for our climate as that of France," adding wickedly, "and some of our British ladies are of the same constitution as the French Marchioness of S., who told the author of the 'Menagiana' how dangerous she found the month of May." In his "Il Pastor Fido," Guarini describes May as—

> Bella madre di fiori,
> D' erbe novelle e di novelli amori,*

The May-day of the *Spectator's* time was that which I call Old May-day, viz., May 12, for it corresponds with May-day at the time when our Bradley "robbed the people," as they thought, "of their eleven days." But if the Old Style had continued till now, our present May 13 would be May-day, for the Julian Calendar caused the dates slowly to pass away from the seasons they had originally corresponded with. It is worthy of notice, however, that the May-day of the last century, though coming later in the true year—that is, the year of seasons—than May-day of Chaucer's time (our May 9), was not likely to be a warmer or brighter day, for from about May 11 to May 14, there usually occurs a singular, and as yet unexplained, "cold snap." On the average, May 9 is at least one degree Fahrenheit warmer than May 12, though our almanacs in giving the mean temperature make no note of this.

The change of style has altered the seasons notably since the days of Spenser, Shakespeare, and Milton. Then Midsummer fell at the very end of June, Midwinter at the very end of December. In one sense this accorded better with the actual changes of temperature than our present arrangement; for Midsummer is not the hottest nor Midwinter the coldest part of the year. The weather was far more likely to be cold and bitter on Old Christmas Day—our Twelfth-night—than it is on our present Christmas Day, when, indeed, the weather is as often soft and warm as bleak and cold. The last week of December was as apt in Shakespeare's time as in the second week of our January to be a time—

> When icicles hang by the wall,
> And Dick the shepherd blows his nail,
> And Tom bears logs into the hall,
> And milk comes frozen home in pail.
> When all aloud the wind doth blow,
> And coughing drowns the parson's saw,
> And birds sit brooding in the snow,
> And Marion's nose looks red and raw;
> When blood is nipp'd and ways be foul,
> And nightly sings the staring owl.

The old May-day ceremonies would show that a marked change had taken place in our seasons, if the May morn of former days fell in the same part of the year of seasons as our present May-day. Chaucer tells us, in his "Court of Love," that early on May-day "forth goth al the court, both most and lest, to fetche the flowris fresch, and braunch and blome." Even Kings and Queens rose early on May-day, to fetch green boughs or May boughs. This is probably the "rite of May" referred to in the "Midsummer Night's Dream,"—"No doubt," says Theseus, "they rose up early to observe the rite of May." On our May-day not many trees are green, but a few days at this season make a great difference in the aspect of the woods and fields, so that on Old May-day folks who rise early, and are not troubled by fears of the "great unpaid," might collect a goodly number of "flowris fresch; and braunch and blome."

Our poets, despite the effect of the change of style, which has thrown back May-day a full fifth nearer the spring equinox than it was in Shakespeare's time, still sing in the same strain of the merry month of May and of the delights of its opening days. But May-day is only poetically "the merriest, maddest day of all the glad New Year." The change of style has practically wrought a change of weather. It has brought down the Maypoles. A few chimney-sweeps may still carry round their Jack-in-the-Green; but the green is usually evergreen, not the fresh growth of the new May. We cannot wonder at the

* This line does not refer in the remotest way to the Derby or to new favourites.

melancholy fate of Tennyson's Queen of the May, if, on one of our modern May-days, she—

"Danced about the Maypole, and in the hazel copse,
Till Charles's wain came out above the tall white chimney-tops."

Someone should have explained to the merrymakers that the sun had not yet attained a mid-day altitude which could justify these gambols in the hazel copse—that, in fact, they would probably result in what Sidney Smith calls "our British constitutional coughs, sore throats, and swelled faces." His advice would have been scouted, no doubt, and he would have been looked upon as a bore and a nuisance; but he would have been right all the same. Indeed, speaking seriously, in these days, when many, especially the younger folks, look back longingly on old customs, and occasionally try to revive them, it may sometimes be well to inquire even into such dull, dry details as the effects of the change of style in modifying the relations of dates and seasons.

CRYSTALS.

By WILLIAM JAGO, F.C.S., Assoc. Inst. Chem.

THE object of this and the succeeding papers of this series is to place before the readers of KNOWLEDGE an account of the Chemistry and Chemical Physics of different subjects of general interest in as plain and interesting a manner as is possible to the writer. It must not be supposed, however, that the result will be a regular and succinct chemical treatise; those requiring such can find them in abundance, and of all prices and qualities. The writer would rather treat his science so that they who run may read, appealing, he hopes, to those who, though busy in the shop, the field, the mart, have not ceased to take an interest in Science for her own sake. A very clear and interesting account of the properties of matter in the gaseous state, which appeared in one of the earlier numbers of KNOWLEDGE, led to an account of its behaviour in the solid condition being chosen as the subject of this introductory paper.

Let us start with the inquiry, "What is a crystal?" and probably at the outset we shall be met with an answer which, curiously enough, selects as its typical example the very substance of all substances that a chemist instances as the type of non-crystallised bodies. Nine persons out of ten will tell us, in response to our query, "A crystal is anything which is very clear, *like glass*." Go to the chemist or geologist and ask him whether a piece of that black, shiny lava poured out of a volcano is crystalline, and he will say, "No, it is a specimen of the glassy (vitreous) type of rock." Here, then, is one point at which the chemist at once joins issue with the popular idea. Let us in the next place ask him for his definition of a crystal, and we learn that almost every solid substance known has a tendency to arrange itself in a definite geometrical form, and that it is then said to be crystallised. This change of bodies from the condition in which their particles are arranged irregularly, to that in which each occupies a stated position, forming part of one harmonious whole, is frequently accompanied by remarkable changes in the appearance and properties of the substance. Carbon, known to every one in the humble and familiar guise of charcoal, crystallises and becomes a diamond; and further, this depends on its particles arranging themselves so as to form the figure known as an octahedron; with a change of the conditions, the same elementary substance varies the crystallised form it assumes, and instead of the hard and brilliant gem, we have produced six-sided plates of soft and metallic-looking graphite, or black lead. Nature carries on within her laboratory these wonderful changes, giving us little or no idea of what she is doing unless we watch and question her closely; her secrets are, however, readily revealed to him who questions aright. Owing to the value and beauty of the diamond, efforts have been made, from the days of the alchemist downwards, to obtain it artificially; these efforts have, however, met with but little reward. Nature has one element of success in her experiments which we can never have, and that is time. The diamond, sparkling in a lady's ring, no bigger than a pea, has, in all probability, taken for its growth not days or months, but years, reckoned by thousands, or perhaps hundreds of thousands! How, then, shall we imitate her? In this particular instance, probably in no way; but there are fortunately other substances known which crystallise more readily, and with these we may hope to have success. Most of the metals assume, under certain conditions, a crystalline form, and those particularly which are found native occur frequently as crystals. The Latrobe nugget, at present in the Natural History Museum, is a magnificent instance of crystals of gold; it consists of natural golden cubes, welded, as it were, together in one mass. Among the metals, bismuth is remarkable for its tendency to crystallise, and by following the directions given, a crystalline mass of bismuth is readily obtained. Take about a quarter of a pound of the commercial metal and melt it either in a small clean iron ladle or over a Bunsen lamp in a porcelain crucible; when quite melted, set the ladle or crucible on a cold metal surface. Let it remain perfectly still, and watch the bismuth carefully, until it is seen to solidify round the edges, then quickly pour out the metal still remaining liquid, and you have the whole of the interior lined with more or less perfect cubical crystals of bismuth. There is one striking peculiarity about these crystals, however. They are but skeleton crystals; the lines forming the edges of the cubes are there, but there is a depression in each face of the crystal evidently not as yet filled up. The growth of the crystal was arrested by pouring out the still liquid metal, and there we have not only shown us the shape of bismuth crystals, but also the manner in which the crystal grows.

For purposes of comparison, try now to make sulphur crystals. To do this, melt down roll sulphur in the ladle or crucible, using, however, a very gentle heat, and not prolonging it beyond the point at which the whole of the sulphur is melted; allow to cool in the same manner as with bismuth, wait until a crust has formed over the surface, and then immediately bore two holes through with a red-hot wire, the one for the liquid sulphur to run out, and the other to admit air. Pour out the sulphur still remaining liquid, and cut carefully round the upper crust with a penknife, remove it, and the whole of the interior is interlaced with delicate needle-shaped, amber-like, crystals of sulphur. Here, then, are two substances, of widely different appearance and properties, both possessing in common this property of crystallising, but with each there is a definite shape. Further experiment and observation teach us that the form of a crystal is as characteristic of a body as any other property it possesses. In the next paper the writer purposes to give further directions for the preparation of crystals, and hopes to add sketches of crystals as viewed by the microscope.

IN an early number, probably the next, an important series of papers by Miss Amelia B. Edwards, the eminent authoress and Egyptologist, on the question, "Was Rameses II. the Oppressor of the Hebrews?" will be commenced.

XUM

THE COMET.

WE continue the path of the comet Wells to the end of next week.

POPULATION OF THE EARTH.

By the Editor.

SEVERAL correspondents ask whether a computation, such as that made in letter 391, p. 575 might not be based on conditions more in accordance with known facts. I suggest the following:—From the Registrar-General's return just presented to Parliament, it appears that for an estimated population of rather more than 26 millions in England, the deaths were, 491,813; the births, 883,508—an increase of 391,695,—say 390,000 for 26,000,000 (the real increase *slightly* greater). This is at the rate of 39 for 2,600, or 3 for 200—that is $1\frac{1}{2}$ per cent. Suppose now that 5,000 years ago the human population of the globe were one million (which, considering that according to every Egyptologist of standing, the Pyramids were built long before that time, and that 30,000 lives were *expended* in building them, must be considered a very moderate allowance), then, what would be the present population of the globe if there had been an increase of $1\frac{1}{2}$ per cent. per annum during the last 5,000 years?

The problem thus presented is very simple. The rate of increase per annum is from 1,000 to 1,015, so that we have merely to multiply one million (or whatever the population was 5,000 years ago) by $\frac{1015}{1000}$ five thousand times. Without logarithms this would be rather a difficult task; but with the aid of logarithms (see our papers on that subject) it is sufficiently easy:—

$$\log. \left(\frac{1015}{1000}\right) = \log. (1015) - \log. (1000)$$
$$= 3{\cdot}0064660 - 3$$
$$= 0{\cdot}0064660$$

Multiplying by 5000 we get,

$$\log. \left(\frac{1015}{1000}\right)^{5000} = 32{\cdot}330000$$
$$\log. \ 1{,}000{,}000 = 6$$

log. (earth's population under the assumed conditions) ..... $= 38{\cdot}330000$

Whence the earth's present population would be

213,800,000,000,000,000,000,000,000,000,000,000,000

if, being only one million 5,000 years ago, it had increased during the last 5,000 years at the same rate that the population of England has increased during the last year. If, 5,000 years ago, the population of the earth had been but 10, we should have to strike five digits from the above.

But, to show how very far the conditions attained in a civilised country are from those under which life could possibly exist for any length of time (as science measures time) on the earth as a whole, let us suppose that only 4,000 years ago (and even the believers in the Great Pyramid as a stone Bible will allow so much), the earth's population did not exceed 10. Then we have, to determine the earth's present population, this calculation:—

$$\log. \left(\frac{1015}{1000}\right)^{4000} = 25{\cdot}864000$$
$$\log. \ 10 = 1$$

log. (earth's population under the assumed conditions) ... ... $= 26{\cdot}864000$

whence the earth's present population would be

731,140,000,000,000,000,000,000,000.

Now we have seen that with ten persons to the square yard (rather close crowding) there would be 30,000,000 to the square mile, and to the entire surface of the earth (land, water, and ice) 200,000,000 times as many, or

6,000,000,000,000,000.

Wherefore, even at the rate of increase which has taken place in this much maligned old country during the past year (of course the "good old times" were ever so much better), the earth's population, beginning with only 10,

four thousand years ago, would now suffice to closely pack the surfaces of about

122,000,000,000

such globes as this earth on which we live, or the surface of one large globe having a diameter exceeding the earth's nearly 5,000 times, or the sun's much more than 400 times. Yet in science 4,000 years count almost as nothing.

When we remember the rate of increase which is customary among those whose lives are easy—this being certainly much nearer 10 than 1½ per cent. per annum—we see that, so far as the benefit of the greater number of surviving persons at any epoch is concerned, it is, on the whole, perhaps as well that during the last 4,000 years there have been certain destroying agencies at work to prevent the population of the earth from increasing so fast as it otherwise might have done.

It may, perhaps, be interesting, before we conclude, to inquire what would have been the actual average increase per cent. per annum if the population of the earth had increased from 10 four thousand years ago, to 1,500,000,000 now. If r be the increase per cent. per annum, we have

$$\log.\left(\frac{100+r}{100}\right)^{4000} = \log.\ (1{,}500{,}000{,}000)$$
$$= 9{\cdot}1760913$$

whence, since $\log. 100 = 2$ we have

$$4000 \log.\ (100+r) = 8000+9{\cdot}1760913$$
$$= 8009{\cdot}1760913$$
$$\log.\ (100+r) = 2{\cdot}0022940$$
$$100+r = 100{\cdot}5304,$$

or the average rate of increase, on the assumptions made has been little more than ½ per cent. per annum. As it is certain that the population of the earth 4,000 years ago consisted of many millions, the real average rate of increase must have been very much less even than this.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

THE constellation Böotes, at which we now arrive (Map, p. 474), will be found a very mine of objects of interest by the incipient observer. We will begin by turning our instrument, armed with a power of 160, upon ϵ, a star which Struve well described as "pulcherrima" (or most beautiful). So viewed it will be seen as in Fig. *b*, p. 511,

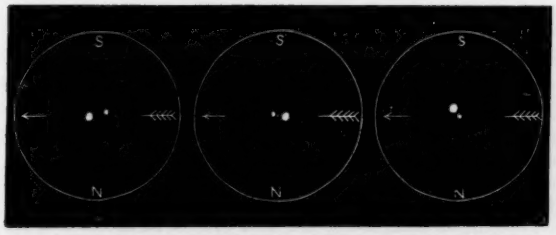

Fig. 27.—π Böotis. Fig. 28.—44 Böotis. Fig. 29.—39 Böotis.

the larger star being yellow, and the companion a bluish-green. π Böotis, an interesting and easy pair, when viewed with a power of 160 will be found to present the appearance shown in Fig. 27. ξ Böotis, is a little closer and somewhat more unequal pair, the colours of the components, moreover, being more strongly contrasted than in the case of the previous star. It is shown in Fig. *a*, p. 511. It is a wide and easy pair, which it is needless to figure. 44 Böotis, shown in Fig. 28, as seen with a power of 160, is interesting from the contrasted colours of its components. It is not numbered in the map, p. 474; but is one of two small stars forming a triangle with β and θ Böotis in it. Nor is our next object, 39 Böotis, numbered; but it is the north-western of the pair of stars in the map, and will be found in the sky, a little above and to the right of 44. In this, again, the colours are prettily contrasted. Its aspect as viewed with the same power as the preceding objects is represented in Fig. 29. κ Böotis, on the confines of Canes Venatici, is a wider and much more unequal pair. It is shown in Fig. 30. On a line drawn from Spica Virginis to ζ Böotis, and about 11° south (and a little east) of Arcturus, will be found the very pretty and interesting double star which we have

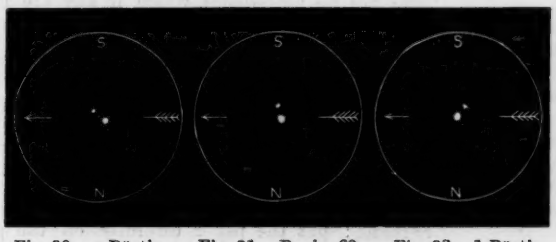

Fig. 30.—κ Böotis. Fig. 31.—P. xiv. 69. Fig. 32.—1 Böotis.

drawn in Fig. 31. It is 69 of the fourteenth hour of Piazzi's catalogue. The difference in colour of the components of this pair will at once strike the observer. He will, though, probably be puzzled to say exactly what the colour of the smaller star is, very discrepant conclusions on this subject having been arrived at. Some 8½° to the west, and just to the north of Arcturus, we shall find a very beautiful object, the star 1 Böotis, shown in Fig. 32. At the first glance the student will observe two stars, nearly of the same magnitude, and wide apart. It is the upper, or southern one of them, to which our attention must be directed. Looking at it carefully, we shall note the minute blue star shown in our figure to the south and very slightly to the east of its primary. We have omitted the second large star of which we have just spoken from our diagram, inasmuch as, using the scale to which it is drawn, such star would be just out of the northern, or lower, portion of the field. Finally, the student may, if he likes, look at ζ Böotis with the very highest power at his command; but, under the most favourable circumstances, he will only succeed in so far converting it into an egg-shaped object as to show that it is not single. Such are a few of the most easily-identifiable objects in this constellation. The number of purely telescopic double stars is very large indeed, but their necessary absence from our map of reference, and the impossibility of recognising them without an equatoreal provided with graduated circles, renders the mere mention of them here sufficient.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART VI.

THE advantage of the negative form of photograph is that from it we are enabled to obtain an almost unlimited number of copies on paper, having the lights and shadows, as in nature. The paper used for the purpose may have a plain *matt* surface, that is without gloss, or it may have a surface of albumen. The plain paper is used

when the photograph is to be coloured. Both kinds may be purchased ready for use, excepting that it is not sensitised.* The necessary appliances, such as printing-frames, porcelain dishes, and other articles, which will be named in due course, it is assumed will have been supplied in the "outfit." We now require a solution of nitrate of silver, and this must be prepared in the proportion of 40 or 50 grains to each ounce of water. The quantity required will depend on the size of the pieces of paper to be prepared—say a quarter of a sheet; the dish, therefore, must be somewhat larger, and solution of silver must be poured in to the depth of about half-an-inch; then take the paper by two corners, and place it on the solution so that it will fall down evenly. After a few moments, with a piece of horn or wood, lift the paper by one corner, so that it may be seen that there are no air-bubbles, which, if any are present, may be removed by a puff of breath, or they may be touched. The paper is to be then replaced, and left for about three minutes—not longer. It may then be lifted slowly, and, after draining the surplus solution (this may be facilitated by drawing the paper over the edge of the porcelain dish) the sheet may be suspended to a line by American "clips," or it may be dried at once by fire-heat. Cut the paper, which must be quite dry, to the size required. Place the negative face, or picture side, upwards in the pressure-frame; put the paper carefully on the negative, then put on the back of the frame, the paper being pressed close by springs or screws, care being taken not to apply more pressure than is necessary to keep the paper and negative in perfect contact. The frame is then to be placed in strong daylight; diffused light is preferable to the direct rays of the sun.

While we suppose the printing is proceeding, we may say that the strength of the silver solution should not be allowed to fall much below 40 grains to the ounce, otherwise the prints will be weak. A good plan is to keep a stock of full strength always ready, and always fill up the bottle of solution to replace the portion used. If it should be found that the silver solution becomes discoloured by use, the colour may be removed by the addition of kaolin or China clay; shake well after each time of using the bath, and when the kaolin has subsided, the silver solution will be found to be quite clear, and may be poured off for use when required. Filtration is sometimes desirable. If the surface of the silver has any scum upon it, this is a sure sign that the bath should be filtered, but the scum can sometimes be removed by drawing the edge of a piece of blotting-paper over the solution.

The strength of the solution of silver may be determined with sufficient accuracy by means of the *argentometer* sold for the purpose.

The progress of the print must be watched. It will be noticed that the paper projecting beyond the edges of the negative has changed to a dark tint. If the negative be a *dense* one, this darkening may be allowed to proceed until the paper is nearly black. The frame should now be removed out of the full daylight, and one-half the back of the frame opened, when, on turning back the print, it will be seen whether the printing is deep enough, and it requires some little experience to determine this. The print should be rather over printed, as the fixing will reduce it to some extent. If not printed deep enough, the finished print will appear weak and unsatisfactory. When as many prints as are required are ready, they should be put, one at a time, into water, in order to remove the excess of chloride of silver; after a few minutes' washing, the water may be poured into a jar for the purpose of recovering the silver, and then, after one or two changes of water (if the second washing water appears milky, it still contains silver), the prints are ready for the next operation—toning.

If the prints were to be *fixed* without toning, they would not have a pleasing colour. We now require another solution, which contains chloride of gold. This salt is supplied in small glass tubes containing 15 grains. The tube may be broken and thrown into a bottle containing 15 ounces of water; there will therefore be one grain of gold to each ounce of water. The use of this solution of gold is for the purpose of changing the colour of the print; the change produced being a combination between the gold and silver in the paper, the gold giving a better colour to the finished print, and the gold-toned print is also more permanent.

There are many kinds of toning-baths, but the one which gives good results, is easy to make and use (it may be used as soon as made); it consists of one ounce of the solution of gold, and, say, 20 ounces of water poured into a porcelain dish. A small piece of *litmus* paper put into the solution will show that it is acid. A lump of carbonate of soda (common washing soda) may be put into the solution, and stirred about until the litmus-paper shows that the solution is slightly alkaline. The prints may now be put into this alkaline gold solution, face downwards, a few at a time, and they must be kept in constant motion, to insure equal toning. As soon as the prints present a pleasing colour, they may be removed into a dish of clean water, and when all are toned, they are ready for the next operation—fixing. It may be remarked here that one grain of gold will be sufficient to tone a sheet of paper; therefore the proportion to be used for each batch of prints must be regulated accordingly.

The *fixing* is effected by means of hyposulphite of soda, in the proportion of 3 ounces of the salt to 20 ounces of water. The prints must be left in the soda solution about 20 minutes, and they must be kept in motion for a time, to insure even action in fixing. When fixed, the prints must be removed to a dish of water, and the water must be frequently changed during the first half-hour. The prints may now be left during the night in the water, and the following day, after several changes, they may be placed between clean cloths or blotting-paper, and if they have been previously *trimmed*, they are ready for mounting.

* If preferred, paper may be purchased ready sensitised, but we assume the amateur will wish to prepare his own.

THE COMING TRANSIT OF VENUS.

By R. A. Proctor.

I PROPOSE to give a short and simple account in these pages, in the next few weeks, of the circumstances under which transits of Venus occur, explaining the general principles on which the determination of the sun's distance by observation of Venus in transit depends. But, as many questions have been addressed to me respecting the places where the whole transit—its beginning and its end—will be seen, it appears well to give at the outset, for immediate reference, a chart showing from what parts of the earth the transit can be seen wholly or in part.

The following preliminary explanation may be useful, however:—

Venus circuiting around the sun in a smaller orbit than the earth, and completing a circuit in 224·7 days, whereas the earth takes 365¼ days, passes between the earth and sun at intervals of about 583·9 days, or a year and seven months. If she travelled in the same plane as the earth, she could not thus pass between the earth and the sun

without seeming to cross the sun's face centrally. But as her path is inclined to the plane in which the earth travels, she is sometimes slightly on one side, sometimes slightly on the other side of that plane, when she is passing between the earth and the sun, so that sometimes she passes above, or north of the sun, sometimes below, or south of him, and is not seen crossing his disc. When, however, it so chances that Venus comes between the earth and the sun at or near the time when she is crossing from one side to the other of the earth's plane, there occurs a transit. This can only happen, of course, at two certain times in the year—viz., at or near the time when the earth is crossing the line through the sun, along which the two orbits—the earth's and Venus's—intersect. One of these times in the year is about June 8, the other about Dec. 7, and a transit of Venus never occurs except at or near one of these dates.

Now, when one of these transits occurs, certain parts of the earth are suitably placed for seeing, either the whole transit, or the beginning or end of it, while from certain positions of the earth no parts of the transit can be seen.

Let us see how such places are determined :—

Suppose that *e* (Fig. 1) represents the earth, *ss'* representing the sun. In reality, both are enormously exaggerated in dimensions compared with the distance separating them. Suppose a cone, *sks'*, enclosing the earth and sun, in the way shown, to travel round with the earth ; and also another cone, *sls'*, touching both the earth and the sun,

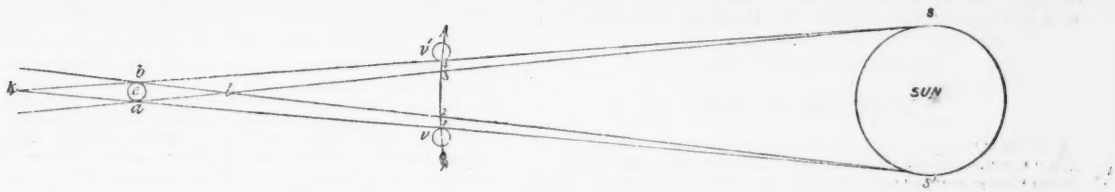

Fig. 1.

but on opposite sides of the vertex *l*. Now, imagine Venus to come along, gaining on the earth, and passing (as she does when there is a transit) through the double cone, at the points 1, 2, 3, and 4 ; 1 being the point where she first reaches the outer surface of the exterior cone ; 4, the point where she finally leaves that cone ; while 2 and 3 are the corresponding points for the inner cone.

Now, a little consideration will show that when Venus reaches the point 1, the transit begins, but only for that particular point where the line from *s'* to 1 produced touches the earth. *There* the transit begins earliest of all. As Venus passes from 1 to 2, transit begins for different places on the earth, until when Venus is at 2, transit begins from just that point where the line from *s'* to 2 produced touches the earth. The face of the earth turned towards the sun during the short time (less than half-an-hour), during which Venus passes from 1 to 2, does not change much ; so that one may say that over a hemisphere of the earth the beginning of the transit is seen, but for about half of that hemisphere the beginning is seen earlier and over the other half later than from the earth's centre (if we could imagine an observer, stationed at that inconvenient spot, to be able to watch the transit).

After passing 2, some six hours elapse before Venus comes to 3, when the end of the transit occurs at its very earliest for the station where *s*3 produced touches the earth ; and then, as Venus passes from 3 to 4, the end is seen at different stations on the earth ; until, finally, when Venus reaches 4, the end is seen at its very latest from that point where the line *s*4 produced touches the earth. Then the transit is over : (and no more transits of Venus will be seen till the year 2004.)

While Venus is between 2 and 3, she is visible on the sun's disc from every part of the earth's surface, turned sunwards, and the portion whence she is thus seen at some time or other is of course more than a hemisphere, seeing that when Venus is at 2, a hemisphere of the earth is turned sunwards, and a different hemisphere when Venus is at 3.

Now, in the large chart there are shown :—

(1) The semi-circles bounding the region (approximately a hemisphere) whence the beginning of the transit is seen. These are marked "transit begins at sunrise," "transit begins at sunset."

(2) The semi-circles bounding the region (approximately a hemisphere) whence the end of the transit is seen. These are marked "transit ends at sunrise," "transit ends at sunset."

Over the region common to both (1) and (2), the whole transit is seen, except in the small region (shaded in the chart), where, though both the beginning and end are seen, a part of the mid-transit is not seen. Over the region belonging to neither (1) nor (2) no part of the transit is seen, except in two small regions (shaded in the chart), whence, though neither the beginning of the transit nor the end is seen, a part of the mid-transit is seen.

Over region 1, a series of dotted lines are drawn, showing how much the Ingress of Venus, or the beginning of the transit, is hastened or delayed (Ingress Accelerated or Retarded, marked "I A so many minutes," or "I R so many minutes").

Over the region 2, a series of dotted lines show how much the Egress of Venus, or the end of the transit, is hastened or delayed (E A or E R).

Then, over the region whence the whole transit can be seen, a series of heavy lines show how much the duration exceeds (+) or falls short of (−) the average.

The places where the duration is greatest and least are marked by heavy black dots, beside one of which is marked the maximum amount of shortening of the duration. (The other being on a part of the earth whence none of the transit can be seen, is not so marked.)

The map is very easily interpreted :—

Thus, in England, say London, we see that only the first part of the transit can be seen, and not much of that, the transit beginning shortly before sunset : ingress is retarded about five minutes. At New York the whole transit can be seen : ingress is retarded nearly eight minutes, and egress accelerated rather more than seven minutes, duration falling short of the mean nearly fifteen minutes.

And similarly for any other places where either the beginning, the end, or the whole transit can be seen.

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XUM

Reviews.

"A RIDE ACROSS THE CHANNEL"*

ALTHOUGH Colonel Burnaby's balloon ride was not intended for scientific observation, this book has interest for the student of science, besides being a graphic account of a balloon ride. The greatest height attained by Colonel Burnaby in this ride was about two miles, and at that elevation the temperature was four degrees below the freezing-point, while at an elevation of 500 feet, a few minutes before, the temperature had been 48° in the shade. The account of the balloon's descent suggests, as usual, that balloonists might with advantage devote some of that ingenuity to devise safe ways of bringing a balloon to rest on the ground, which has hitherto been fruitlessly expended on attempts to guide the balloon through the air. As a scientific statement, Col. Burnaby's assurance respecting the effect of the balloon's apparition on hens is open to question. "Thank Heaven I have seen it," cried a middle-aged female. "It passed over my house like the dome of a cathedral; and all my hens are still in convulsions of fright at its appearance." On which the advocate of Cockle's Antibilious Pills gravely assured her that "the apprehensions of her hens would not diminish, but rather increase, their laying powers." He should have added that all eggs thereafter laid by those hens, would be marked with a balloon in full career "like the dome of a cathedral."

FLOWERS IN MAY.

AT the opening of May, by far the commonest buttercup in our meadows is the bulbous species (*Ranunculus bulbosus.*) It grows almost everywhere. Buttercups as a group may be always easily recognised by pulling out the petals, when they will be seen to have a small hollow scale near the base. The bulbous kind is known both by the way its calyx is turned back tightly against the stalk, and by the rough bulb formed by the lower part of the stem. As the month wears on, the tall meadow buttercup (*R. acris*) becomes commoner in the fields: its calyx grows in the normal fashion, enclosing the petals, and the middle division of its leaves starts from the same point as the outer ones. A third species found almost as universally is the creeping buttercup (*R. repens*), exactly like the last in most respects, but with rooting runners and the central leaflet on a separate stalk, apart from the two-side leaflets. The water crowfoot (*R. aquatilis*) has white flowers and finely-divided submerged leaves with larger floating ones: it is common in shallow, muddy water. The ivy-leaved crowfoot (*R. hederaceus*) differs from it only in the absence of the submerged leaves; it creeps on mud beside the water. Of the rose family, two or three little potentillas may be found abundantly. They have yellow flowers, and may be roughly recognised by their double calyx. The tormentil (*P. tormentilla*) has only four petals; it grows on high, windy places. The cinquefoil (*P. reptans*) has five petals and five divisions to its strawberry-like leaf. Silver-weed (*P. anserina*) has similar flowers, but many little leaflets arranged in two rows on each side of a long stalk. Both these are road-side weeds. Herb-bennet (*Geum urbanum*) much resembles the potentillas in flower and in its double calyx, but is a taller and weedier plant, with little hooked fruits forming a sort of rough burr. It is common in hedgerows. The hedge itself blossoms now too; its hawthorn bushes are also roses by family; notice their single calyx. The pinks are another family well represented this month. As a rule, most of the small white flowers growing in ordinary situations, with a single capsule in the centre of the flower, filled with seeds arranged centrally around an axis, are almost sure to be pinks. The smaller kinds have the calyx in separate pieces. Of these, mouse-ear chickweed (*Cerastium vulgatum*) has five split petals, and a capsule funnily cocked up at the end and opening in ten teeth. It grows everywhere. Common chickweed (*Stellaria media*) has flowers much the same in appearance, but its capsule opens in five valves, and it may easily be recognised by a single line of hairs running down one side of the stem. Sandwort (*Arenaria trinervis*) looks very like the last, and can only be discriminated by its petals, which are entire instead of being two-cleft, and by the absence of the line of hairs. The larger pinks have the calyx united into a sort of cup or tube. Two of them are common this month—red campion (*Lychnis diurna*), which is pink, with scentless flowers; and white campion (*L. vespertina*), which is white and scented. Ragged Robin (*L. flos-cuculi*) is very like the first-named, but has much-divided petals, and a less swollen calyx. The veronicas are another group that can be well studied in May. They have blue flowers, the petals united into a tube at the base, four lobes to the corolla, and only two stamens. These peculiarities will at once distinguish them from any other English plants. Some of them have the flowers arranged in leafless spikes starting from the axils of the leaves. Two such may be found in May: *V. chamædrys*, the germander, with hairy leaves and two lines of long hairs, one on each side of the stalk; and *V. beccabunga*, brooklime, with smooth leaves and hairless stem. The first haunts road-sides, the second running streams. Another set of Veronicas has the flowers solitary in the axils, not in spikes. One such, with little shining leaves and tiny white blue-streaked blossoms, growing among grass in fields, is the thyme-leaved speedwell (*V. serpyllifolia*); another, with ivy-shaped leaves, is the *V. hederæfolia*; a third, with the upper leaves reduced to mere long, lance-like bracts, is the wall veronica (*V. arvensis*). Two others, very common in fields, are Buxbaum's and the procumbent speedwell. They may be known from the others by their broad, toothed leaves, not ivy-shaped, and by their upper leaves like the lower ones, only smaller, but they are harder to distinguish from one another. Buxbaum's (*V. Buxbaumii*) has a capsule twice as broad as long; the procumbent speedwell (*V. agrestis*) has it about the same breadth as length. We have only room for one other family, the orchids, known by their spur and their tuberous roots, as well as by their curious twisted ovary. The green-winged orchis (*O. morio*) may be recognised by its green-veined sepals; it is a southern plant only. The military orchid (*O. militaris*), with a handsome spike of purple-red flowers, and a long, two-cleft centre lobe to the lip, belongs only to the counties around London. The male orchis (*O. mascula*), with a pair of spreading sepals, as if winged for flight, is over early in the month. The spotted orchis (*O. maculata*) with lobed tubers and a very dense spike, lasts through the whole of May. Altogether, several hundred plants flower in May, and of these at least a hundred and fifty are common everywhere, so that it is necessary to make a selection; but whoever masters these five groups to start with, will have done a good month's botanical work.

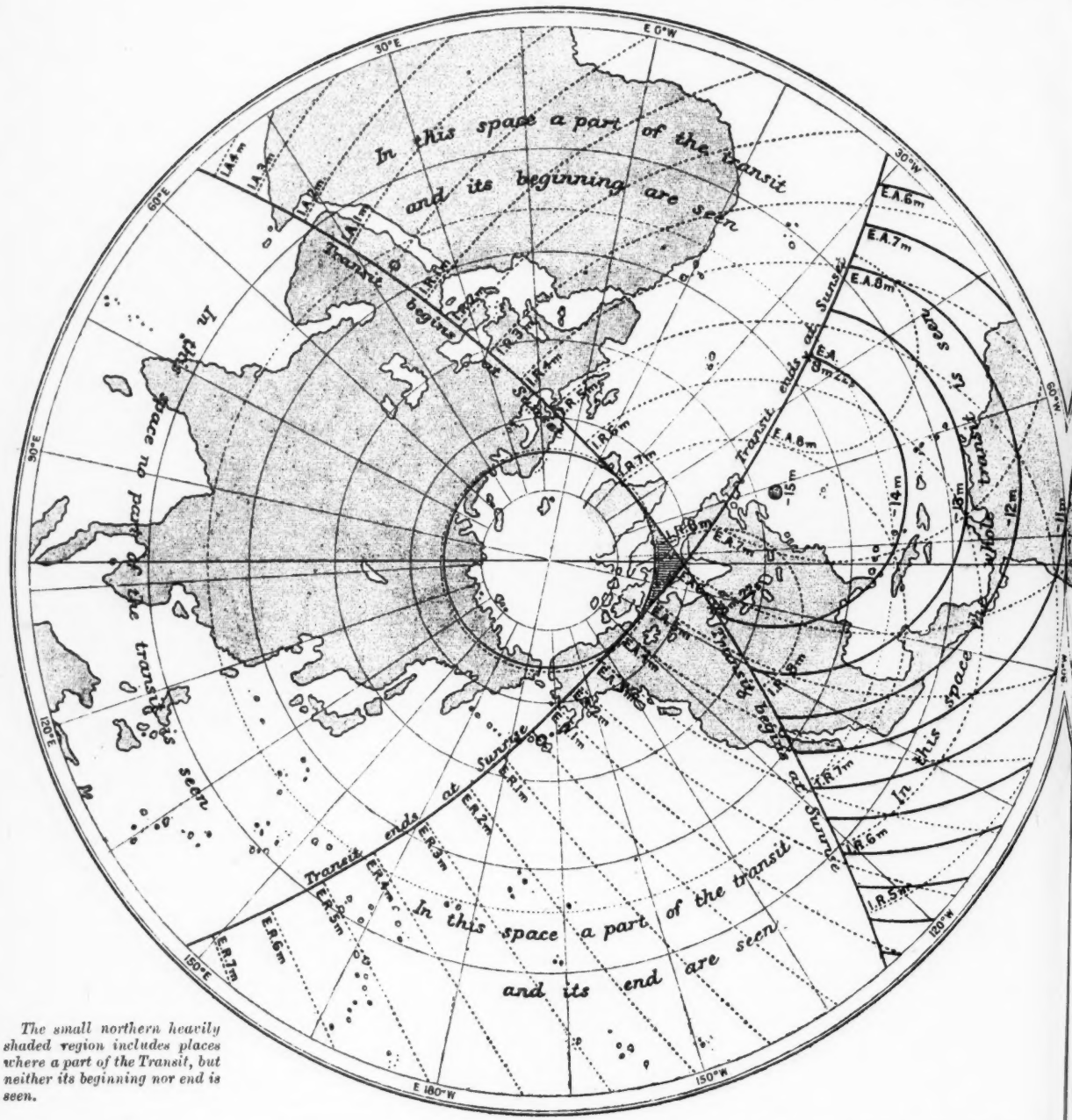
SOLAR APPARATUS.—It will be remembered that M. Mouchot, a short time ago, devised an apparatus for utilisation of solar heat, and that M. Pifre made some important improvements on it. Very different views have been taken as to the practical utility of such an apparatus. Some help towards a right judgment now comes from Montpellier, where a French Government Commission has been experimenting with the apparatus for a year (1881). Another commission has experimented at Constantine, in Algeria, but the results are not yet published. The apparatus was of the known form—a concave mirror, with blackened boiler in the axis, surrounded by a glass envelope. The steam from the boiling water was condensed in a coiled tube cooled by water. The weight of water distilled in an hour indicated the amount of heat utilised; and observations with an actinometer from hour to hour showed the amount of incident heat. The rates of these two quantities was a measure of the economic efficiency of the apparatus. The temperature and moisture of the air, &c., were also carefully noted. The number of days of observation was 177, and of observations 930, and water was distilled to the amount of 2,725 litres. Without entering much into numerical detail, we may state that while the heat utilised in the most favourable circumstances per square mètre per hour would be about equal to that utilised from 240 grammes of coal (supposing about a half to be utilised)—even the half of this is not attainable in our climate. The sun does not shine continuously enough for practical utilisation of the apparatus. In very dry and hot climates, the possibility of utilisation would depend on various circumstances, such as the degree of difficulty of procuring fuel, the price and facility of transport of solar apparatus, &c. We note in the report (by M. Crova) that the efficiency of the apparatus is not proportional to the heat intensity of the solar radiations, and hardly ever varies in the same sense. The absolute quantity of heat utilised, on the other hand, depends essentially on the temperature of the air; the higher this is, and the less consequently the cooling, the greater the amount of heat utilised.—*The Times.*

* "A Ride across the Channel, and other Adventures in the Air." By Col. Fred Burnaby. (London: Sampson Low, Marston & Co. 1882.) One Shilling.

XUM

CHART OF THE TRANSIT OF 1882, ON TH

Showing the lines of equal acceleration and retardation of ingress and egress (.....)

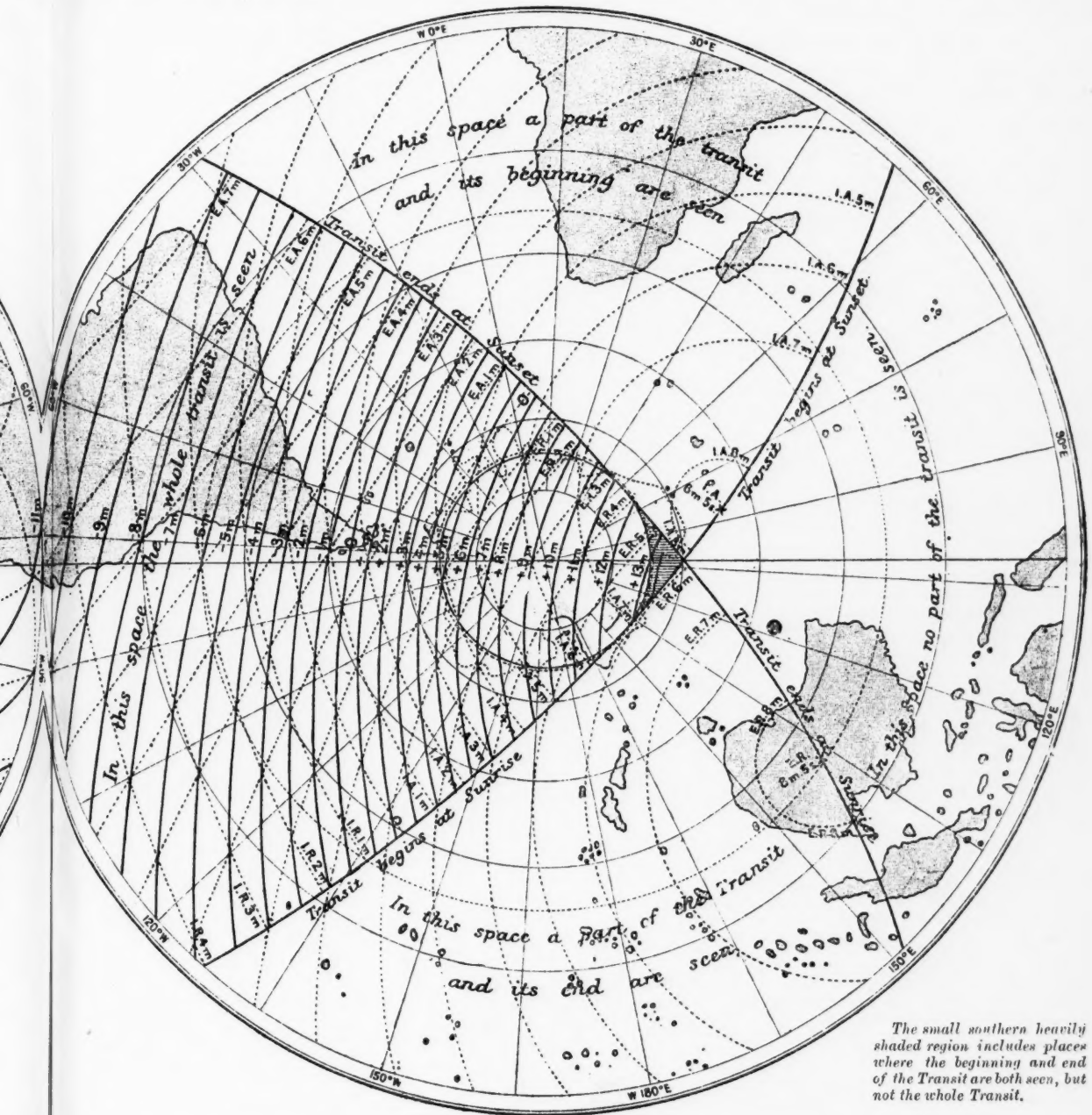


The small northern heavily shaded region includes places where a part of the Transit, but neither its beginning nor end is seen.

ABBREVIATIONS.—I.R. 5m. = Ingress Retarded 5m.; E.A. 6m. = Egress Accelerated 6m.; +7m. = Du

ON THE STEREOGRAPHIC PROJECTION,

ress (.....) ; and also the lines of equal duration (——) for external contact.



The small southern heavily shaded region includes places where the beginning and end of the Transit are both seen, but not the whole Transit.

n.; + 7m. = Duration 7m. greater than mean value; — 8m. = Duration 10m. less than mean value.

XUM

WEATHER REPORT, FOR WEEK ENDING SATURDAY, MAY 6.

STATION.	ABERDEEN.							LIVERPOOL.							VALENCIA.							LONDON.							LYONS.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max.	50	55	51	52	55	54	56	47	50	52	58	57	54	55	49	61	54	57	54	56	57	54	56	59	61	71	58	65	64	—	54	—	76	63	65
Thermometer in Shade. Min.	39	35	42	36	45	35	40	34	38	42	46	45	42	43	36	37	42	45	36	36	48	37	44	43	47	51	46	49	46	—	46	—	57	48	45
Thermometer in Shade. D. e. y.'s Range	11	20	9	16	20	19	16	13	12	10	12	12	12	12	13	24	12	12	18	20	9	17	22	16	14	20	12	16	18	8	8	8	19	15	20
Barometer. Inches	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	38	—	30	—	29	29	30
Barometer. 100ths	·34	·53	·60	·88	·61	·80	·83	·61	·51	·73	·83	·69	·84	·92	·51	·22	·70	·73	·78	·88	·90	·80	·72	·92	·82	·65	·86	·88	·10	—	·11	—	·77	·96	·01
Wind. Direction	N W	S	S	S S E	S S W	N N W	S S W	W S W	S S E	S S E	E	W	S S E	N	S S W	S S W	W	N N E	—	—	N W	S S W	S S W	S S W	E N E	—	S S W	S S W	E	E S E	—	S S W	S W	S S W	S S E
Wind. Force (0 to 12)	6	4	6	2	4	3	3	3	3	2	2	2	2	2	6	7	3	3	0	0	1	4	4	3	3	0	2	4	1	1	1	1	1	1	1
Weather. Beaufort Scale*	c	b	b c	b	b c	b	o	b	o	b c	m	o	b c	b	c p	q p c	b c	o p	b	b c	c	b b c c	b	o	g r m	o	r	b	—	—	—	—	o	o	b
Rain-fall. 100ths of inch.	3	0	12	4	30	0	0	105	9	14	2	16	0	20	34	59	10	12	0	0	38	21	12	4	0	15	12	8	8	—	35	—	0	51	0

* WEATHER.—*Beaufort Scale is*, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

SCREW-DRIVER TUBES.

[395]—I am afraid screws would not take to my friend Mr. Ellis's spiral tubes as kindly as ulcers do, even though it has since been extended (as he doubtless knows) so that you can drill a hole in a tooth—and the cheek behind it—in no time, with your foot, by a drill at the end of a long spiral of that kind, which will work round any bead. But I do not write merely to say that, but to add to my original mode and simple slate-pencil tube, that it may be made of steel, and have three slits, besides the opening in the lower 2 inches of it, and the pieces may then be flattened and bent outwards a little, enough to hold the largest screw-head. A ring slipped over them will bring the fangs together, enough just to fit smaller screws. Whether you see your screw or not, you can very soon feel if it is going crooked—the feeling is too enraging to mistake for a moment. I observe (in both senses) your notice about brevity of correspondence.

EDM. BECKETT.

CONSERVATION OF SOLAR ENERGY.

[396]—I give now in full Dr. Siemens's letter referred to at p. 566 last week, omitting only those references to Mr. Archibald, to whom he was replying, which do not belong to the letter, regarded as a reply to my own reasoning in the *Cornhill Magazine*, and at pp. 565-566 of KNOWLEDGE.

Dr. Siemens's letter, then, runs thus:—[Mr. Proctor] has missed the principal point of my argument concerning solar fan-action. I showed pretty clearly I thought that solar gravitation would affect the inflowing and the outflowing currents equally, and that centrifugal action must determine motion in the equatorial direction in a space filled with matter. But, to put the problem into a mathematical garb, let us consider the condition of two eqnal masses, m_p and m_a, both at the radius R from the solar centre, the one opposite either pole, and the other opposite the equatorial region. The moment of gravitation of both these masses will be represented respectively by $\frac{gm_p}{R^2}$ and $\frac{gm_a}{R^2}$, and supposing both masses to be gaseous, and of the same chemical composition and temperature, they will represent equal volumes, say one cubic foot.

These conditions being granted, we may put—

$$\frac{gm_p}{R^2} = \frac{gm_a}{R^2},$$

but the mass m_a is subject to another force, that produced by tangential motion, which shall be represented by v, and the centrifugal

force resulting from this motion by ϕv; [Why put it vaguely thus, writing "function of v," when we know what the centrifugal tendency is, viz., $\frac{m_a v^2}{R}$?]

the moment of gravitation towards the sun will then be reduced to $\frac{gm_a}{R^2} - m_a\phi v$, and the latter factor being a positive quantity we have—

$$\frac{gm_p}{R^2} > \frac{gm_a}{R^2} - m_a\phi v.$$

This inequality of attractive moments must determine motion toward the sun in favour of $\frac{gm_p}{R^2}$, and this condition holding good for any value of g and R, it follows that the polar inflow and equatorial outflow must take place, provided only that space is not empty, as supposed by Laplace, but filled with either an elastic or non-elastic fluid.

To put it in another way, Mr. [Proctor] imagines that, in order to determine an outflow from the sun, it is necessary for the centrifugal moment $m_a\phi v$ to exceed the moment of gravitation $\frac{gm_a}{R^2}$, whereas according to my view, the value of the former determines only the rate of outflow, but is immaterial as regards the principle of action. The projection of dust is entirely dependent upon the outflowing current. I leave it for Mr. [Proctor] to determine for himself the velocity of current necessary to move a particle of dust of given size and weight away from the sun in opposition to its force of gravity, which I am well aware is twenty-seven times that of the earth on its surface.

The gaseous current is of course produced at the expense of solar rotation, but this expenditure of energy is relatively much smaller than that lost to our earth through tidal action, and may be neglected for our present purposes. It is, moreover, counterbalanced by solar shrinkage, as explained in my paper.

C. WM. SIEMENS.

[I fear Dr. Siemens' way of treating this question is but too correctly described by himself as putting it in a mathematical garb, for there is only the garb of mathematics, not the thing itself, in the above discussion, and even the garb is not quite correct. For instance, in mathematics the term "moment" is not used as in Dr. Siemens' letter, nor in any way even approaching to his use of the term. One can tell, of course, very clearly what Dr. Siemens means, and therefore, perhaps, it is unimportant whether he correctly expresses his meaning or not—except as showing that discussions of this kind are somewhat outside the usual course of his inquiries and reading (and also, what was, however, already known, that the Editor of *Nature* has no very profound mathematical knowledge). Turning, then, to Dr. Siemens' meaning, I note that, in the first place, proving that a cubic foot opposite the pole and another opposite the equator are unequally attracted towards the sun's centre, by no means suffices to prove that either will move in any particular way. It would be the tendencies of *neighbouring* cubic feet we should have to consider, not those of two cubic feet hundreds of thousands of miles apart. Inflow of a mass of vapour opposite either pole would depend on the state of the gaseous matter immediately below it, and it can very readily be shown that the pressures which would exist opposite the polar regions, and the consequent resistance to inflow, would be greater, not less, than at equal distances in equatorial directions. But the chief objection to Dr. Siemens' reasoning (I was about to call it specious, but it is not so) lies in this, that he considers a certain consequence which would not even follow at all, as though it not only would certainly follow, but having followed, would leave things as they were, so far as the circumstances causing inflow and outflow are concerned. Under the impossible conditions he describes, equilibrium would be unstable (though he does not, as I conceded for the sake of argument last week, prove this), and movements tending to restore equilibrium would accordingly take place; but Dr. Siemens assumes, in effect, that there will be no tendency towards equilibrium, but that the forces tending to produce motion will remain all the time unchanged. It is as though having shown that the water forming the hollow of a wave tends to rise, one were to assume that it will rise for ever.

What Sir John Herschel said of the theory that the Zodiacal light is "a solar atmosphere in any proper sense of the word" is true of Dr. Siemens's suppositious atmosphere, "the existence of such a gaseous envelope propagating pressure from part to part subject to mutual friction in its strata, and therefore rotating in the same or nearly the same time with the central body, and of such dimensions and ellipticity, is utterly incompatible with dynamical laws." The case is certainly not strengthened by reasoning which, while endeavouring to show that the more aggregated parts of the supposed atmosphere have such a figure as is attributed to the outer corona and the zodiacal, assumes nevertheless the possibility of equal densities at equal distances opposite the polar and equatorial regions.

Mairan's views involved rather an excess than a deficiency of centrifugal tendency, and what Laplace did was to show not that a solar atmosphere would extend no further than a certain distance under any conditions, but that no such atmosphere could, beyond a certain distance, share in the solar rotation, without being entirely freed from any tendency sunwards. This does not seem to be what Dr. Siemens supposes to have been Laplace's reasoning, seeing that his views would be rather supported than opposed by such freeing of gaseous matter to travel outwards.

Again, there is all the difference in the world between the effect attributed to solar rotation in constantly expelling gaseous matter throughout an enormous extent of space around the sun, and that tidal action which affects the earth's rotation, not by the actual motion of the ocean, but by the mere transmission of wave states.

However, the points touched in the two preceding paragraphs are relatively insignificant.—RICHARD A. PROCTOR.]

CONSUMPTION.

[397]—Anything coming from so brilliant an intelligence as that of Professor Tyndall deserves attention. In this instance, at any rate, he has cut before the point. His conclusions do not justify his premises, his premises his conclusions. They remind one of a statement of his made a few years back, that the air of a sick chamber, by passing through cotton wool, might be made pure as the air of the Upper Alps, forgetful that the carbonic acid of respiration could not be thus eliminated, and the atmosphere in so far rendered pure. Professor Carpenter, going on some statements of Professors Villemin and Klebs, informed us that tubercle was owing to a micrococcus or microphyte, a little berry or little plant. Now, we are asked to believe that it depends on a little stick or *bacillus*, as Professor Koch terms some presumed organism. These inquirers, however, are led away on a wrong scent. If there be a micrococcus or *bacillus* met with in tubercle, it is an occurrence entirely fortuitous, and in no way essential to the production of tubercle itself. I have had as much to do with tubercle as most persons, and I never saw any micrococcus, any *bacillus*. Tubercle is wholly unorganised, in fact, a *caput mortuum*, consisting of the unoxidised carbonaceous waste, not excreted by reason of insufficient congress with the oxygen of the atmosphere. Not only tuberculous matter, but almost any extraneous substance is capable, upon inoculation, of producing tubercle in subjects predisposed, so that it is quite unnecessary to torture animals in order to verify this position. The general spread of tubercular disease does not depend upon inoculation, but on the respiration of prebreathed air. If we only avoid prebreathed air, tubercle and tuberculous disease become impossible. At least a fourth of the human race are reputed to perish tubercle-stricken.—I am, sir, your obedient servant,

HENRY MACCORMAC, M.D.,
Consulting Physician to the Royal Hospital.

[We have inserted Dr. MacCormac's letter, though the *Times* would not, because in such matters the fullest discussion is desirable. Albeit there is one point to which I must take exception. Professor Tyndall never made the mistake attributed to him by Dr. MacCormac. I remember perfectly well his first popular statement of the action of cotton wool in his lecture on dust and disease, and I am sure not a person in the lecture-room supposed for a moment that he meant what Dr. MacCormac implies, viz., that the air was otherwise purified than by the elimination of organic matter. Dr. MacCormac, in his enthusiasm for the theory which he enunciated in his book on the "Breath Rebreathed," seems able to close his eyes to facts which most students of minute life consider demonstrative. When Dr. Koch not only saw with the microscope (what Dr. MacCormac has not seen) the minute *bacilli*, but has succeeded in developing and as it were rearing, generation after generation of *bacilli*, it is rather too much to ask science to reject all belief in these organisms. Nor can we see how the crucial experiments, described so lucidly by Professor Tyndall (KNOWLEDGE, p. 547), can possibly be controverted by any number of experiments showing (what no one doubts) the bad effects of breathing prebreathed air.—ED.]

[Several interesting letters on "Consumption" are unavoidably held over.]

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Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

X. K. You will see that the opponents of Dr. Koch's conclusions use arguments which, if accepted, would render fresh experiments necessary. However, there can be no question you are right in asserting the general principle, that to repeat experiments by which a truth has already been established is simply cruelty. Only doubt can justify the renewal of experiments of the kind.—W. STRINGFIELD. We have posted your letter to "George," and have sent your communication to the publishers. Thanks.—BOYD MOSS. We thank you; but the discussion of the general question of vivisection is not suited to our columns. In company with Professor Tyndall's interesting communication about consumption, we printed his incidental allusion to the necessity of certain experiments; but that does not open up a discussion on a subject already overworn. I am sure neither Professor Tyndall nor any true student of science would advocate other than such carefully-arranged experiments as you describe.—ALEX. SCOTT. The paragraph you referred to was borrowed through another source; we named the original source, the "Journal of the Society of Chemical Industry," but we do not know the publishers. The address, "Publishers of the Journal," &c., would, we imagine, be sufficient.—CONSTANT READER AND RECOMMENDER. Thanks. The quotation was made by a correspondent, and seemed so suitable that we added it to our set. If incorrect we must remove it, for as corrected it does not seem so suitable: the motto, "Orthodoxy is God's Truth," is open to misconception.—P. W. CLAYDEN. The eclipse to which you refer was that of Sept. 7, 1820, well seen, as a partial eclipse, throughout the British Isles.—W. DALE. Letter received, and shall appear.—C. J. WATSON. The theory is quite untenable; everything known about the action of solar heat, as of solar light, indicates radiation, and everything known about the sun indicates that its globe is intensely heated.—CANADENSIS. It is a mere accident that some articles of mine are inserted as "By the Editor," others by "R. A. Proctor." The "Answers to Correspondents" are not all by the same hand. I write most of them, because no one else would be willing to wade through so much of the correspondence which reaches the office of KNOWLEDGE. "Conducted by R. A. Proctor" means "edited," or rather includes it and something more.—DESMOND. Enlarging KNOWLEDGE, and making the price 6d., might have the effect of increasing the "circulation among the higher and superior educated branches of society," but it would put it beyond the reach of many to whom I wish to be of use. Those who will not take a paper *because* it only costs twopence would hardly help us much. No; our plan was to make KNOWLEDGE as low-priced as possible, and to give as much as we possibly could for the money. To that plan we must adhere. To create a circulation, and after bringing our paper into request, to increase the price of it, would not be fair.—W. GADD. I fear it will be impossible to find space.—C. CARTER BRAINE. Will ask engraver if he can manage it. "Brake" is correct, but some prefer "break."—JOS. OFFORD. Many thanks. Will use as soon as possible.—ANOTHER HATER OF SUPERSTITION. No fear of a theological controversy; but why should you object more to the term Almighty than to the term Infinite? What *are* natural causes and events, but those operations which, being finite, we can understand, or hope to understand? Outside of them lies the infinite. To speak of infinite time may be idle, seeing that we cannot conceive either; but any one may do so without offence; he may equally speak without offence of Infinite Power. There is nothing necessarily theological in the conception.—ERIN GO BRAGH. Fear Otto of Roses somewhat outside our line.—E. H. B. STEPHENSON. It has been by noting changes among the stars that the sun's motion in space has been recognised; but the changes are too slight for ordinary observation. Some of them have only been detected after centuries of observation. Tyndall's book on "Dust and Disease" (Longmans) would suit you, I think; but the subject is very wide. Thanks for lines, but fear some would object to them as having rather a goody-goody tone.—A. C. H. If that were so, how could a ship sail close to the wind? Estimating as you do the driving force of the wind, the action of a wind dead abeam would produce no way at all, except leeway, and a wind six points (67½ degrees) from a ship's course would drive her astern. You can only resolve the wind's action as I have done, viz., parallel to the sail and square to it, then this last part must be resolved in direction of ship's course and square to it. The wind thus dealt with must be the relative wind, viz., that which seems to blow when the ship is travelling on her course at whatever rate she may have at the moment.—T. P. GARRETT. Thanks, but it reaches two members of staff.—SUBSCRIBER TO KNOWLEDGE. We can give no information as to either advertisement. No telescopes procurable at the office of KNOWLEDGE, or sold directly or indirectly through the agency of publishers or proprietors of the paper. Regret that no correspondent has been able to reply to question about sulphur cast (query 266, Feb. 24).—A CHURCHMAN. I should imagine the Rev. Dr. Liddon would be a better judge than you can be on that subject. But then, your letter being anonymous, you may answer that as I do not know who you are, I cannot tell whether you are a judge or not. After all, what can it matter to you, being so sure as you are of your opinion, whether others think differently or not?—S. C. GAUBERT. The generalisation is bold; certainly so much has not yet been proved.—CYCLOID. Certainly, the lowest point is for the moment at rest, the uppermost moving twice as fast as the circle's centre. You ask, why? The answer is, because at the lowest point the advancing motion of the circle as a whole is exactly counterbalanced by the (there) receding motion of the circumference due to rotation. At the uppermost point both these motions are in the same direction. If v be the rate at which the centre advances, and also that at which, were the centre at rest, a point on the circumference would be moving on account of rotation, then the velocity of the lowest point is $v-v$, or 0, while that of the uppermost point is $v+v$, or $2v$.—SOLARIUM. The correct angle for the gnomon of a sundial to be erected on Streatham Common would be 51½°. There may be a minute or two of difference between 51° 31′, the latitude of London, and that of Streatham, but it would be impossible to make a gnomon so precisely as to take this into account. The best way to place the style is by the sun, not by the pole star. At true solar noon (which you can get from "Whitaker's Almanack," if you have true clock time, by adding or subtracting the equation of time according to the date), the style must have its shadow in its own plane. Thus, on May 12, see "Whitaker's Almanack," p. 34, the sun is 3m. 52s. before the clock, so that the sun is due south at 3m. 52s. to twelve clock time. The style can then be set with its plane vertical, and due north and south, the slant edge, of course, pointing towards the pole of the heavens, or due north, 52½° above the horizon.—A CONSTANT READER. We must not trespass on the province of medical men. A surgeon who has seen the formation would know better than correspondents who had not; and we should be afraid of advice being given which might lead to mischief.

[About two pages of "Answers" have been unavoidably held over.]

Our Mathematical Column.

THE LAWS OF PROBABILITY.

BY THE EDITOR.

THE law enunciated at the close of the last paper enables us to determine the probability that a certain series of results will follow, in a certain definite order, when any definite *trial*, as a tossing, drawing, or the like, is repeated such and such a number of times; but it does not tell us what the probability is that so many results out of the total number will be of one kind, without regard to order. For example, suppose there are three white balls and seven black balls in a bag, and that we draw a ball five times, always at random, and always returning the drawn ball. Then, the chance that the drawings give, first a white ball, then three black balls, then a white ball, is as follows:—

$$\frac{3}{10}\times\frac{7}{10}\times\frac{7}{10}\times\frac{7}{10}\times\frac{3}{10}; \text{ or } \frac{3087}{100000}$$

—the odds in fact are more than 30 to 1 against such a result. But the probability that two drawings out of the five will give a white ball and that three drawings will give a black ball is very different. The odds are against such a result; but they are not nearly so heavy as against the former. This is easily seen; because the particular succession above considered is only one out of several results which would give two white drawings and three black ones. If we consider in how many ways this proportion of white and black drawings may be brought about, we shall be led to recognise the true method of determining the probability of this result.

Call a white drawing w, and a black drawing b. Then the above particular result is represented by the arrangement, $w\ b\ b\ b\ w$. But

algebra tells us that out of two w's and three b's we can make $\frac{1.2.3.4.5}{1.2 \times 1.2.3}$ different permutations. Now, we have seen that the chance of any given one of these occurring is

$$\frac{3}{10} \times \frac{3}{10} \times \frac{7}{10} \times \frac{7}{10} \times \frac{7}{10}; \text{ or, } \frac{3^2.7^3}{10^5};$$

hence, obviously, the chance that some one or other of these permutations occurring is obtained by multiplying their total number into the probability of the occurrence of one out of that number. This gives, as the required probability, $\frac{1.2.3.4.5}{1.2 \times 1.2.3} \times \frac{3^2.7^3}{10^5}$; which may either be reduced into the form $\frac{4.5}{1.2} \times \frac{3^2.7^3}{10^5}$ (for further reduction), or may be conveniently written, $\frac{\lfloor 5}{\lfloor 2 \cdot \lfloor 3} \cdot \frac{3^2.7^3}{10^5}$; its value is $\frac{3087}{10000}$.

If we notice how this result has been obtained, we readily deduce the following important law:—If, at each of a set of $(n+m)$ trials, there are $(p+q)$ possible results, all equally likely, p being of one kind and q of another, then the probability that n results will be of the former kind, and m of the latter, is

$$\frac{\lfloor n+m}{\lfloor n \, \lfloor m} \cdot \frac{p^n q^m}{(p+q)^{\lfloor n+m}}$$

I give a few illustrations of the application of this law, before proceeding to notice how the expressions representing these probabilities are related to certain well-known algebraical theorems.

Suppose we wish to determine the probability that in tossing a coin eight times there will be five heads and three tails. Here p is 1 and q is 1; n is 5 and m is 3. So that the required probability is—

$$\frac{\lfloor 8}{\lfloor 3 \quad \lfloor 5} \cdot \frac{1}{2^8} \text{ that is } \frac{6.7.8}{1.2.3} \cdot \frac{1}{2^8}; \text{ or, } \frac{7}{32}.$$

So that the odds are 25 to 7 against five heads and three tails being tossed.

If we required the odds against five tossings being of one kind and three of the other, without caring whether heads or tails showed oftenest, we must obviously double the above probability, since there must be exactly equal chances for the result five heads and three tails, and for the result three heads and five tails. Thus, we get as the chance that five tossings will be of one kind and three of the other $\frac{7}{16}$, or the odds 9 to 7 against such a result.

Now let us inquire what the chance is that the eight tossings will give four heads and four tails. Our formula gives in this case—

$$\frac{\lfloor 8}{\lfloor 4 \quad \lfloor 4} \cdot \frac{1}{2^8}; \text{ or, } \frac{5.6.7.8}{1.2.3.4} \cdot \frac{1}{2^8}; \text{ or, } \frac{35}{128}.$$

So that the odds are 93 to 35 against such a result. (The reader will readily see why there is no doubling in this case.)

Observe that $\frac{35}{128}$ is greater than $\frac{7}{32}$, but less than $\frac{7}{16}$; so that when a coin is tossed eight times, we are more likely to get four heads and four tails than *either* five heads and three tails, *or* three heads and five tails; but we are more likely to get one of these two last results than the first result.

What, however, is the chance that six heads and two tails will result?

Our formula gives

$$\frac{\lfloor 8}{\lfloor 2 \quad \lfloor 6} \cdot \frac{1}{2^8}; \text{ or, } \frac{7.8}{1.2} \cdot \frac{1}{2^8}; \text{ that is, } \frac{7}{64}$$

the odds are therefore 57 to 7 against such a result.

The chance that six tossings will be of one kind and two of the other is $\frac{7}{32}$.

It is similarly shown that the chance of seven heads and one tail being tossed is $\frac{1}{32}$; the chance that seven tossings are of one kind and one of another being $\frac{1}{16}$.

The chance that all the tossings give head is $\frac{1}{256}$; the chance that all are of one kind is $\frac{1}{128}$.

We notice, then, that the most probable number of heads is four; and in like manner the most probable number of tails is four; but the most probable assortment of heads and tails is such that there will be five of one kind and three of the other.

It would follow, therefore, that if two persons of equal fortune were to venture half their fortune on each of eight successive tossings, the most likely of all results is that one or other will be just ruined at the end of the series of tossings. But it is equally likely that one or other will be the loser; and it is rather more likely that they will come off quits than that one of them (specified beforehand) will be ruined. This supposes that all the eight tossings are completed before accounts are cleared; and therefore the policy of gambling is somewhat too favourably treated; for clearly two unfavourable tossings to begin with, or three unfavourable out of the four first tossings, although they might be cancelled by favourable throws if the tossing were continued, would yet complete the ruin of a player, if the money ventured had to be handed over to the winner after each several tossing.

Let us next take the following example:—

A die is thrown eight times; what is the chance that ace is thrown twice (exactly)? Here the p of our formula is 1, the q is 5 (since there are five throws other than ace); n is 2 and m is 6. Thus the required chance is by our formula—

$$\frac{\lfloor 8}{\lfloor 2 \quad \lfloor 6} \cdot \frac{5^6}{6^8}; \text{ or } 28 \cdot \frac{5^6}{6^8}$$

the value of which can be easily obtained either by direct calculation or by means of logarithms.

If we examine our formula—

$$\frac{\lfloor n+m}{\lfloor n \, \lfloor m} \cdot \frac{p^n \, q^m}{(p+q)^{n+m}}$$

We find that it can be divided into two parts, each readily defined. First, there is the expression $(p+q)^{n+m}$, which obviously corresponds to the total number of possible results when there are $(p+q)$ possible events at each trial, and $(n+m)$ trials. The other portion

$$\frac{\lfloor n+m}{\lfloor n \, \lfloor m} p^n \, q^m$$

must, therefore, represent the total number of favourable results, that is, the total number of results fulfilling the required conditions. It is easily seen that this is so. For in fact, if we take any particular case in which n of the results are of the kind which can happen in p ways, and m are of the other kind which can happen in q ways, we see that this particular case can be varied in $p^n \, q^m$ ways. For instance, reverting to our illustrative case, the particular result $w \, b \, b \, b \, w$ may be varied by having any one of the three white balls to give the first w, by having any one of the seven black balls for the first b, any one of the same set for the next b, and so on; giving in all 3 times, 7 times, 7 times, 7 times, 3 possible variations in which the sequence is $w \, b \, b \, b \, w$—that is, $3^2 \, 7^3$ such variations. And the number of possible sequences of $n+m$ results, of which n are of one kind and m of another, is, by a well-known rule,

$$\frac{\lfloor n+m}{\lfloor n \, \lfloor m.}$$

Hence the total number of favourable results is obtained by multiplying these numbers together, or is,

$$\frac{\lfloor n+m}{\lfloor n \quad \lfloor m} \cdot p^n \, q^n.$$

This expression is the term involving $p^n \, q^m$ in the expansion of $(p+q)$ to the power $(n+m)$.

So that our law may be thus expressed. If there are $n+m$ trials, at each of which some one of $p+q$ events, all equally likely, must occur, p of these events being of one kind and q of another; then the chance that n events will be the former kind and m of the latter, is represented by the fraction of which the numerator is the term involving $p^n \, q^m$ in the expansion of $(p+q)$ to the power $(n+m)$, the denominator being the complete expansion.

This is the chance that there will be *exactly* n results of the former kind. The chance that there will be *at least* n results of the former kind is obviously obtained by adding together for the numerator all the terms of the expansion from the first down to the term involving $p^n \, q^m$ (both inclusive), the denominator being, as in the former case, the complete expansion.

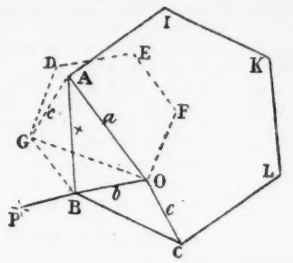
* The symbol $\lfloor$ implies that all the whole numbers, from one up to the number indicated within the symbol, are to be multiplied together.

MATHEMATICAL PROBLEM.

[41]—Show that a proper fraction, which in its lowest terms takes the form $\frac{a}{b \cdot 2^m \cdot 5^n}$ (a and b being prime to each other and to 10), when converted into a decimal, recurs from, but not before, the $(m+1)^{th}$ or $(n+1)^{th}$ digit after the decimal point, according as $m >$ or $< n$.—GRADATIM.

["Gradatim" supplies a solution of the above problem, turning on the lemma that if a is prime to b, the fraction $\frac{a}{b}$ = a decimal recurring from first digit after point. I would submit that writing the fraction in the form $\frac{a}{b} \cdot \frac{5^{m-n}}{10^m}$ or $\frac{a}{b} \cdot \frac{2^{n-m}}{10^n}$ according as $m >$ or $< n$, the above result follows at once from this lemma.—ED.]

[42]—Given the length of three lines, a, b, and c, drawn from any point within a regular polygon of n sides, to any three of its consecutive corners, A, B, and C. Required a geometrical determination of the polygon, granting that the polygon of n sides can be constructed when one of its sides is given.—Y.



Our Chess Column.

Endings from actual games contested by Leonard P. Rees.

POSITION No. 40.
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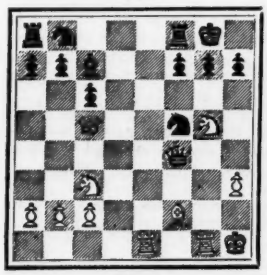

WHITE.
White to play and win.

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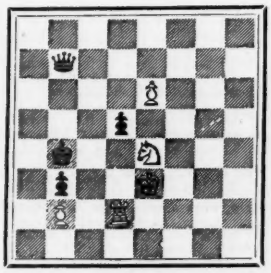

WHITE.
White to play and win.

HIGHGATE CHESS CLUB.

President — Professor Tomlinson.

CONSULTATION GAME, PLAYED MARCH 6, 1882.

Kings' Gambit.

White.	Black.	White.	Black.
1. P to K4	P to K4	11. P to K6	P takes P (e)
2. P to KB4	P takes P	12. B takes P(ch)	B takes B
3. Kt to KB3	P to KKt4	13. Q takes B(ch)	K to Rsq
4. B to B4	B to Kt2	14. Kt to QR3	Q to Q4
5. Castles	P to Q3	15. Q to KKt4	QKt to K4
6. P to QB3 (a)	P to KR3	16. Kt takes Kt	Q takes Kt
7. P to Q4	Kt to K2 (b)	17. B to Q2	QR to Qsq
8. Q to QKt3 (c)	Castles	18. QR to Ksq (f)	Q to QB4(ch)
9. P to K5 (d)	P takes P	19. Resigns.	
10. P takes P	QKt to B3		

NOTES.

(a) P to Q4 deserves a slight preference; it enables White to choose different lines of play, in addition to his having the option of arriving at the position in the text by following with P to B3. The attack obtained in this variation of the King's Gambit is very indifferent.

(b) His best.

(c) Sounder than the usual move of P to Kt3, as Black, by replying to the latter move with P to Kt5, will obtain a good game.

(d) Here we think P to Kt3 might have been played with more safety than before; there is nothing to be gained by P to K5, which weakens White's centre.

(e) This is good enough, but Black might also have played P to B4 in order to be able to win; the P on K6 play would, however, have become more difficult.

(f) This was an oversignt; he now loses the Bishop. He might have played B to Ksq with a view of playing B to B2 and getting his Rooks into play, and also bringing his Queen's Knight into active operations. Black had an extra Pawn, but his King's side was exposed, and White still had some chance of retrieving his fortunes.

The following bright game illustrates the attack obtained by 10. KKt to Kt5 in the Giuoco Piano:—

White. Leonard P. Rees.	Black.	White. L. P. R.	Black.
1. P to K4	P to K4	12. Q to R6	Q takes P
2. Kt to KB3	Kt to QB3	13. R to Qsq	Q to K4
3. B to B4	B to B4	14. P to B4 (e)	Q takes BP (f)
4. P to B3	Kt to B3	15. Q to Kt7	Q to K6(ch)
5. P to Q4	P takes P	16. K to Bsq	Q to B5(ch)
6. P takes P	B to Kt5(ch)	17. Kt to B3	Q to B4 (g)
7. B to Q2	B takes B	18. Q takes R(ch)	K to K2
8. QKt takes B	Kt takes KP	19. K to B2	P to KR4 (h)
9. Kt takes Kt (a)	P to Q4	20. Kt to B6 (i)	Q takes Kt
10. KKt to Kt5 (b)	P takes B (c)	21. KR to Ksq(ch)	B to K3
11. Q to R5	P to KKt3(d)	22. R to Q7(ch)	Resigns

NOTES.

(a) As we have shown in our analysis of the Giuoco piano, p. 442, White can continue with 9. P to Q5, obtaining thereby a very fair game.

(b) In the same analysis we characterised the move as an attacking style; it may become very dangerous, but, with correct defence, it will prove less effective.

(c) This is exactly what nine players out of ten will do, but we demur to this move. White will obtain a strong attack by Q to R5. Black can prevent this by just delaying the capture of the piece for one move, and playing instead 10. B to B4. It is obvious that the key move of White's attack, Q to R5 would be bad now, as Black would reply with B to Kt3. Q to K2 would be met by Black's castling. 10. B to B4 destroys White's attack entirely, and, in our opinion, even gives Black a superiority; for supposing now B takes P, Q takes B, White's Queen's Pawn must eventually fall.

(d) Q to K2 would be met by White Castling, with a good game.

(e) An attacking move; 14. Castles would also have been good play.

(f) Black's position is very precarious; 14. Q takes KtP would have prevented for a time the entry of the White Queen on Kt7. 14. Q to R5(ch) would have been met by 15. Kt to QB3, and, if Black then proceeded with 15. B to Q2, with the idea of Castling, White would play 16. Kt takes BP.

(g) White has played very well. Black is now compelled to give up his Rook, as otherwise he would lose his Queen, *i.e.*, 17. R to Bsq, 18. Kt to B6(ch), 18. K to K2, 19. Kt to Q5(ch).

(h) R to Ktsq was better, as he could then play B to K3.

(i) This wins the Queen; KR to Ksq, however, looked stronger.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Leonard P. Rees. — Best thanks for Endings, which were ingeniously played. Solutions of Nos. 36, 37, and 38 correct.

G. Licence. — Solutions correct. Problems received will be examined. Thanks for good wishes.

H. A. N.—Problem received with thanks.

Brenton.—Solution of 35, 36, and 38 correct.

A. McD.—Solution of 38 incorrect.

J. B. B.—Solutions of Nos. 35, 36, and 37 correct.

Correct Solution of No. 38 received from G. W. Edward Wilson, Fusee, Moleque.

XUM

Fusee.—You are quite correct in your explanation.

H. J. Barker.—W. W. Morgan, 23, Great Queen-street, sells pocket chess-boards. It is quite "straightforward" to draw by perpetual check; in some cases even highly creditable.

Correct solution of Problem No. 39 received from Moleque, Fusee, Alfred B. Palmer, Soc, J. Wrigley, J. P., H. A. L. S., H. A. N.

Solutions of Nos. 38 and 39 also received from J. Griffith, J. B., of Bosford.

J. C. Royle *v.* J. Griffith.

Our Whist Column.

By "Five of Clubs."

A GAME FOR STUDY.

THE HANDS.

A.
Clubs—K, 10,9,8,7,6,2.
Spades—K.
Hearts—K, 4.
Diamonds—K, Kn, 9.

B.
Clubs—A, Kn.
Spades—Q, 9.
Hearts—Kn, 10, 9, 8, 7.
Diamonds—7, 6, 4, 3.

Y.
Clubs—Q, 4, 3.
Spades—Kn, 8, 5.
Hearts—A, 5, 2.
Diamonds—A, Q, 10, 2.

Z.
Clubs—5.
Spades—A, 10, 7, 6, 4, 3, 2.
Hearts—Q, 6, 3.
Diamonds—8, 5.

Score.—*A B*, 3; *Y Z*, 4.

Players are invited by a correspondent to test these hands (which will be familiar to readers of Cavendish) by playing them in the usual way, and report through our columns—with the original lead in each case—the results of their game.

Solutions of Problem IV.—H. L. L., K. C., Corrigan, and others correct. We propose to publish the solution next week.

A Two-Suit Hand.

A correspondent, W. H. G., points out correctly that a two-suit hand, 7 of one suit and 6 of another, may be formed in twice as many ways as we have indicated in our discussion of such hands at page 561. Our reasoning assumes that each set of six from one suit may be combined with each set of seven from another, or *vice versâ*, whereas for *or* we should read *and*, thus of course doubling the actual number of such combinations. Thus, instead of the odds being 53,911 to 1 against such a combination outside of trumps, we should have obtained 26,955 to 1; and instead of 35,940 to 1 for such a combination in any suit, we should have obtained 17,970 to 1. W. H. G. suggests also, but with less confidence, that instead of considering that there are 51 cards out of which a combination of the former kind may be made, we should have taken the whole pack. Let us consider this point. Any player but the dealer is to have thirteen cards dealt to him, and the question is, what is the chance that these thirteen cards will consist of seven of one suit, six of another, neither suit being that to which a certain card in the dealer's hand, the last he deals himself, belongs? Now if we consider these conditions (more carefully than I did in replying at p. 561 to my correspondent's query), we shall see that it is not the particular card which the dealer turns up last which should be excluded, but one suit, which has been already done. In fact, the question really is this, What is the chance that a given set of thirteen cards taken at random will be six of one suit, seven of another suit, out of three suits? The chance of this is just half the chance that the thirteen cards will be six of one suit, seven of another suit, out of all four suits; hence the odds against are rather more than 35,940 to 1, instead of 26,955 to 1.—Ed.]

J. Montague.—It is the same problem. Mr. Clay's treatise appeared in 1864; but, in the latest edition, the wording is unaltered. Doubtless, had he been alive, he would not have allowed the words "a few months back" to stand.—Five of Clubs.

"WHIST FOR BEGINNERS."*

It is somewhat singular that when a very small and elementary book is to be written, the writer seems to think the occasion one for being discursive. In this little book, with only twenty-seven very small pages, there are more wasted words and sentences than in Cavendish or Pole. The mistake is made, in fact, of supposing that the best way to explain matters for beginners is to wander round and round the point as long as possible. For instance, where, except in a ten-volume treatise on whist, ought the writer to expatiate like this over the suits:—

"When the beginner has sorted his cards into the four suits, he will greatly assist his memory if he will try to consider his hand as containing only four units, each suit being treated as an unit of more or less strength. It is difficult to remember all the thirteen cards in detail; but if each suit is treated as an unit of strength, the memory has to deal primarily with only four things instead of thirteen. It is something like treating each suit as a separate regiment; if you call your trump suit artillery, and your long suit, *i.e.*, the suit in which you hold most cards, cavalry, the other two suits are your infantry. This metaphor may seem strained, but whist is a battle, and you must at once begin to attack or to defend yourself." More than a page taken up in saying what might be said in two lines.

If, with all this palaver, the rules for correct whist play in all ordinary cases were properly given, we might still be content. But they are not. Thus, the only rule given for a suit headed by Ace King is this—if you have Ace and King and three small cards in a suit, you should lead the King first; and similarly with King Queen; as though with Ace, King, and two small ones, or King, Queen, and two small ones, the lead should be different. Here again is a general rule which is enough to make Clay rise from the grave:—"As the game progresses, you may find yourself obliged to lead from a suit in which you hold only three cards. It is the safest plan to lead the highest of the three cards, as it may strengthen your partner:" the truth being that in a great number of cases your only chance is to retain the best in a three-card suit, as a defence against your adversaries, while in every case in which you have nothing to guide you, the odds are 2 to 1 against your partner being stronger than either adversary in the suit in which you are weak.

Again, instead of the general rule that in returning your partner's lead, you should return the best of two cards left, and lowest of three cards left, Mr. Buckland says, "When you take the trick in the suit led by your partner, you should return," &c., implying not only that when an adversary has taken the first trick, the rule does not hold, but also that you ought always to return the lead when you take the trick in your partner's suit.

The rule given for the discard is also only true when trumps are not declared against you; when they are, the discard should be from longest suit.

A little book like this might contain a great deal of useful and correct information. This little book does not.

* "Whist for Beginners." By C. T. Buckland, F.R.Z.S. (London: W. H. Allen & Co.)

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